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PERCEPTION AND EXPERIMENT

THE deceitfulness of the senses is an ancient theme of philosophy, variations on which have provided the material for many discussions. Some of these are of purely historical interest; but at times they take on new importance when they seem to call in question the methods and results of science, and some form of "philosophy of science" may be called on to restore the balance. The odd feature of the situation is that, just as reliance on perception continues to be the mark of ordinary successful life, so scientific activity seems to go on well enough without recourse to these attempts to justify it. The real difficulties seem to arise when this activity interacts with the general structure of society, producing results which are often unexpected, sometimes unwelcome, and in any case not easily disposed of by general discussions of "scientific method," since the different fields of science seem to have their own ways of dealing with the problem. It may be profitable to look at one aspect of the matter, the nature of experimental procedure, both in its relation to perception and in its bearing on fields outside science where its application has been more or less convincingly advocated.

In a restricted sense, an experiment is the insertion into a situation of some device intended either to sharpen or to extend our knowledge of it, under more or less sharply controlled conditions. The device may be a specific instrument, or it may be a means of producing changes in the matter under observation, perhaps over a considerable interval, thus leading to the employment of recording instruments of various types. As the need of close control is more clearly felt, experiments are carried out in laboratories especially designed for the task, and more and more carefully protected from disturbing influences. An experiment intended at first for the discovery of new facts may be repeated for the verification of those facts without expectation of the revision of the results already obtained, though the possibility of such revision is never excluded, and it may occur with unexpected and startling consequences. Further, an instrument intended for one sort of experiment may suggest application to a material of a different sort, which may yield different results. Such developments are worked out in practice, and

may far exceed anything which the inventor of any instrument had in mind at the outset.

Since not all material lends itself to strict laboratory procedure, some of it must be dealt with by field studies, as in much of biology, or by the use of models which reproduce natural conditions on a smaller scale, as in the study of rivers and shore-lines for erosion and flood-control. In such cases the role of the instrument may become less clear-cut, and it must be largely supplemented by direct observation; but the effort will still be made to keep the two sorts of data as close together as may be. As a middle stage, we may think of animal and plant breeding, where selection of desired traits is largely possible, but where the genetic material may sometimes be intractable, and where the time factor is often of considerable importance. In some cases the experimental outlook is reduced to an exacter knowledge of conditions already recognized, in others it aims at modifications which may ultimately go a long way, so that the exact degree of conscious experimentation has to be decided in the individual case, even though the general attitude is fairly uniform.

If certain conditions can be partially reproduced in small-scale laboratory procedures, others may be studied under conditions not reproducible in the laboratory at all. These latter usually depend on the discovery of new instruments, which provide observations not obtainable by previously available means. An early and striking example is found in the views of stellar constitution developed by Lockyer with the help of the spectroscope. Finding a new line in the solar spectrum, he correctly interpreted it as the mark of a new element; realizing that the stars furnished conditions of heat and pressure not realizable on the earth, he used the grouping of stellar spectra as the basis of a new outline of chemical classification.[1] He could not, however, fully interpret his results, because he did not know that a slight alteration of mass could mean the appearance of a new element, and hence he was unable to give his ideas full cogency, or to win for them the attention they deserved. Conversely, after the electronic theory had been fully accepted, it was equally difficult for physicists to conceive of a heavy atom being split in half, and some years passed before the fact of nuclear fission became an established doctrine.

Instances similar to these could be adduced from various fields, such as the highly charged particles in cosmic rays which are being approached from below by the artificial excitation of particles in the laboratories of Brookhaven and Birmingham, or the discovery that

[1] See *The Life and Work of Sir Norman Lockyer* (1928), *passim*.

radio impulses can be received from outer space in the huge antennae devised for the purpose, thus opening up the new field of radio astronomy. "Even in its earliest beginnings," says a recent writer,[2] "the advance of nuclear physics waited upon the development of experimental resources, the bombarding particles and the means of detecting nuclear radiations. This has remained true." The fact is more spectacular in this field, but the principle holds more or less throughout the history of science. Consequently, control of conditions and accurate adjustment of instruments became of paramount importance, and the laboratory becomes less and less like the outer world, so that it is often questionable how far laboratory techniques and results can be applied in that world, or accepted as the only goal of scientific procedure. It is beyond doubt, however, that the laboratory ideal has, in many fields, been immensely influential, and its results often beneficial.

It is a familiar fact that many instruments which began as purely experimental devices are now in general use for obtaining information about a present state of affairs, without thought of the properties and structures which fit them for that use. When we use a thermometer to measure the temperature outside our window, we are not thinking of the properties of mercury which fit it for that purpose; we want to know what the temperature is, and the thermometer tells us. The point is the information itself, not how it is obtained; and devices of considerable accuracy and complexity may be used by many who do not know how they were built or how they work. An enormous extension of this attitude has come about in recent years, and has certainly had its effect on the general view of experimental procedure and on the estimate of its results, particularly in the disposition to overlook the necessary tasks of production and maintenance which underlie the whole complex situation. After all, instruments do not make and maintain themselves.

In all these procedures the ingredients of experience are on the same level. We may be observing a precipitate or a charge of color in a test-tube; we may be observing the position of a pointer on a scale, or the coincidence of two spots of light in an optical experiment; but we accept the instruments and their behavior precisely as we accept any natural objects we may be dealing with. When a record is made by the observer, or when he notes a graph on a recording device, we still regard the basic data as sufficiently given in sensory experience, and do not pause to ask how they got there. There is no break between the data and our thought about them until we encounter something which provokes uncertainty or con-

[2] H. E. Huntley, *Nuclear Species* (1954), p. 3.

flict among our notions—disappointed expectation, failure of something to behave in the normal way. When a solution which should be acid proves to be alkaline, we conclude that there has been some error in its preparation or in the identification of its components; when a substance which has been put through various processes exhibits new qualities, we inquire into the factors which may have brought about the change. Doubts and uncertainties in such cases arise within the situation, they do not spring from any doubts concerning sensation itself.

All experimental instruments, then, are perfectly normal extensions of our perceptive and manipulative powers, all parts of a single body of experience. Likewise, there is an unbroken gradation from the crudest to the most developed form of any single type of instrument. When Berkeley argues[3] that a microscope "presents us with a new scene of visible objects, quite different from what we behold with the naked eye," and that these objects "have not the like connexion" with the ordinary tangible objects to which we are accustomed, it is hard to see what he can mean. When we examine a printed page through a reading glass, the tangible aspect of the page is not altered; when we examine an object with a microscope, we can adjust its position or otherwise manipulate it without any sense of its being a new object. (It is odd that Berkeley does not consider the case of the telescope which reveals objects of which we have, and probably can have, no tangible experience.) Unless we accept his doctrine of the complete heterogeneity of sight and touch, there is no more reason for saying that we see a different object in the microscope than for saying that we never see an object which is actually before us.

Experimental procedure, then, requires no special treatment to fit it into the fabric of ordinary experience; but its manifestations within it are not all of a piece. It is not easy to draw a line between pure knowledge and application, or between application on a small and on a large scale; it is not easy to extend results secured under the strictest laboratory conditions to the less homogeneous conditions of outside fields; nor is it always easy to be sure what experiments are best worth making in a given context. As a consequence, the connotations of the word "experiment" vary considerably. It may suggest either strict control under limited conditions or the free extension of applications in any direction; it may suggest either scrupulous attention to the matter in hand, with avoidance of further implication, or a bold and untrammeled advance in any suggested direction, perhaps with an indifference to conse-

[3] *A New Theory of Vision*, section 85.

quences. In short, experiment can mean different things in different circumstances; it may mean one thing in the laboratory, and quite another in the fields of political or economic innovation.

The first difficulty in the way of extension of the concept to these fields is that of controlling the initial conditions. In a laboratory, preparation of materials and exclusion of disturbing factors can be carried far; in a political body, material must be taken as it is found, and exclusion of disturbing factors may not be practicable. (How often does taking something "out of politics" mean putting it into a different kind of politics!) Secondly, the material experimented on in a laboratory may be either restored to its original state or replaced by an equivalent; but the body politic remains what it is, and a basic alteration cannot be recalled. An extension in the base of the franchise for instance, is not too difficult to make, but, once made, is almost impossible to reverse, even if expected benefits do not result from it. Likewise, in the case of a managed currency, it may be easy to initiate changes, but very difficult to check their action once they are fairly under way, or to control public reactions to them. Innovation may sometimes be necessary, but there is little virtue in calling it experiment when important constituents of the experimental method are lacking.

A third consideration is the statement of negative or unfavorable results. In theory, and often in practice, a scientist is prepared to state what he has not found out; but in the case of political and economic proposals it is often very difficult to discover how they have actually turned out. I have seen several educational experiments which were announced with much impressiveness but which later faded out with little notice, leaving things much as they were, and certainly with no frank statement that they had not come up to expectations. We can all recall policies of which it was said that "we planned it that way" while they were going well, but which were disavowed when unexpected difficulties cropped up. As good an example as any is the unfortunate British plan to grow ground-nuts in East Africa, which was begun without proper examination of the field or calculation of necessary equipment, carried on with a lavish disregard of expense, and concluded in complete collapse. It is always possible for an undertaking to end in failure; but it is not necessary to give it the supposed dignity of an experiment when the precautions attending a genuine experiment are absent or disregarded.

Thus it appears that we may choose between taking experiment in its strictest sense, in which case it may become inapplicable to ordinary experience, or extending it until it may be applied to almost any activity. In practice, no doubt, we tend toward a middle

position, in which features of the scientific experiment are kept in view so far as possible, and the absence of some of them regarded as of minor importance. One might, perhaps, regard human experience as consisting of a gigantic series of experiments, in which each individual is regarded as an instrument; but it is not clear for what purpose the experiments should be set up, or why there should be so many of them. On the other hand, some biologists are accustomed to speak of nature as experimenting in the production of new species, or to refer to extinct species as "nature's unsuccessful experiments," as if the difference between the few years at most of a human experiment and the millions of years in the life of a species were of no significance. Was a titanothere less "successful" than a rhinoceros because of the date at which he lived, or a rhinoceros more "successful" because he survived to be shot by a human hunter, instead of dying of natural causes before human hunters appeared on the scene?

It is perhaps fair to say that any activity can be carried on in an experimental spirit, if we mean that it should have a definite purpose, be conducted under controlled conditions, and be prepared to entertain correction, or even failure; but all three of these specifications may be held to with more or less sharpness. The builders of motor-cars subject their products to exhausting road tests or even to arranged collisions, in order to discover what they will stand, or where their weak points are; but it would be absurd to suppose that every careless driver who smashes into a utility pole is conducting a similar experiment. Yet it is also possible, by examination of what is left after an accident, to arrive at conclusions about what caused it, and how it might have been prevented, or at least mitigated; and these conclusions are based on the situation as perceived, or compared with other situations similarly perceived. Whatever corrections may later be made, the starting point is always such a perceptual situation, accepted as trustworthy in its main outlines, and communicable to other observers.

In these procedures the mind is not, and cannot be, purely passive. "The senses," says Locke,[4] "at first let in particular ideas, and furnish the yet empty cabinet, and the mind by degrees growing familiar with some of them, they are lodged in the memory, and names got to them." What is this "cabinet"? Not the mind, which grows familiar with what is stored there; not, on Locke's view, the brain. This grocery-store metaphor, so to call it, is singularly unilluminating if applied to the mind directly; an empty store precedes, and can be distinguished from, what is brought

[4] *Essay*, I. ii. 15.

into it, and from its proprietor, and a self-service store does not serve itself, but its customers. The same objection applies to Eddington's picture of the mind as a newpaper office in which "scrappy messages from the outside world" are somehow made up into a presentable story. In either case, the mind seems to become aware of something which is not its own creation or the mere manipulation of its own ideas, as wholly interior to itself. Professor Ryle may have exorcized the ghost in the machine, but we are still obliged to take account of the man in the office.

It has not proved possible to build up a consistent view of experience out of isolated perceptions, whether they are called simple ideas or sensa or anything else. Few of us go around remarking "Redness is here," or trying to intuit a brown patch in its primordial purity. In ordinary life, the closest approach to this is perhaps found in visible or audible signals, such as traffic lights or telephone calls. A traffic light, for instance, may be watched until it changes; but the interest is in the action which the light inhibits or allows, not in the light itself. Similarly, the ring of a telephone may be heard with indifference, if we merely attend to it; with annoyance, if it breaks into something we are intent on doing; or with apprehension, if it is expected to announce bad news. The sound is the same, but the reaction depends on the total situation. "Neutral, indubitable sense-data,"[5] whether or not they can be discovered in experience, are certainly not its main constituents; or, if we look for sensory patterns, we find them embedded in or relieved against the whole background of sensation which, by its changes, keeps the brain active, and without which consciousness would lapse.

Science, then, and the devising and use of scientific instruments, have grown up in perceptual experience, and are subject to the controls of experience. The only way of learning about our sensory apparatus is by observing it; the only way of learning to use an instrument is the way of sensory familiarity. If one set of sensory reports seems at any time in need of correction, that can be supplied either by more reports of the same kind or by a comparison of the reports of different senses. But what we are considering is not merely "ideas in the mind," but the adventures of the mind among things. When, as the result of "thinking about things," we come to feel that some as yet unobserved entity is present in an experience, the natural next step is to go and look for it. Astronomers looked for the hypothetical Neptune, and found it; physicists

[5] The title of an excellent discussion by Lewis E. Hahn, this JOURNAL, Vol. XXXVI (1939), pp. 589–600.

looked for the hypothetical meson, and found it; and when the object sought is not yet found, the proper attitude is one of suspended judgment and further scrutiny. What may not be discoverable in one way may yield to a different approach, or to the application of a different instrument.

Looking at the matter from the opposite direction, we see that our sensory equipment and the instruments which extend it are intimately bound up with the world which science reveals. Two processes which are vital to both life and science—photosynthesis in plants and the chemical response to light in eyes and photographic plates—are explicable only in terms of the quantum of energy.[6] It would be odd if the information which the senses convey about a world in which they have grown and developed should be essentially erroneous and misleading. In practice, of course, no one examines his epistemological conscience before he reads a scientific journal; he takes what he finds there to be no less and no more a part of human experience than anything else. It is strange that much of what calls itself "philosophy of science" should divide its attention between the extremes of the most abstract aspects of mathematical physics and a view of perception amazingly unlike the forms which perception assumes in the lives of ordinary men and scientists alike. What becomes of the whole intermediate range in which materials are developed and shaped to the new uses which have transformed our time?

To put the case specifically, what account would a thinker like Russell give of the activities of a group investigating the uses of such previously neglected metals as titanium, zirconium, germanium, or studying the effects of radio-active isotopes on growth and disease? Such activities are certainly regarded as scientific; but no one, while they are going on, thinks of them solely in terms of waves and particles, or of pointer-readings on unobserved instruments. Again, an infra-red photograph of a distant mountain does not reveal something else, but the mountain as it would appear if various disturbing influences were eliminated—as, by infra-red rays, they are. If we hold that all familiar objects are illusions, we no longer have the right to prefer other objects because they serve some purpose which we think desirable; if chairs and tables must go, so must thermometers and cyclotrons and all the apparatus by which scientists think they are discovering something about the world. Two significant chapters in Pledge's *Science Since 1500* are headed "Real Materials," as opposed to

[6] See L. J. and M. J. Milne, "The Quantum and Life," in *Scientific Monthly*, March, 1951, pp. 139–147.

the "perfect" solids, liquids, and gases postulated by mathematical physics. The workers who deal with such materials have hardly felt themselves to be dealing with illusions, or acted as if they thought they were.

It used to be said that the key-words of science were prediction and control; today they could be replaced by precision and productivity. Both measurements and operations have been carried to a pitch of accuracy previously undreamed of; products both natural and artificial have multiplied even to excess, and the records of observation and experiment are taxing our ability to store and to utilize them. It is an expansion far beyond the scope of eighteenth-century, or even nineteenth-century, philosophers, and ancient and desiccated epistemologies are not fitted to help us understand it. There are urgent practical problems, such as the increasing cost of research and publication, the distribution of new medical techniques, the disposal of radio-active wastes, which are not likely to await the arrival of a more refined theory of perception and knowing. Science, in a myriad of forms, touches ordinary life at countless points, and neither, as things now stand, can be fully taken account of without the other. The task, one would think, is large enough to absorb all the energy we can bring to bear on it.

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PROCEDURAL AND MATERIAL RULES

THE distinction between material and procedural rules has figured prominently in the work of a number of recent philosophers and has long been important for legal theorists. Yet in some ways it is either unclear or unfit for the uses to which it is being put. In this paper I shall discuss some of these uses, some of the ways in which the distinction has been made, and some of the difficulties involved. I shall propose a new form of the distinction designed to avoid these difficulties.

I. USES TO WHICH THE PROCEDURAL-MATERIAL DISTINCTION HAS BEEN PUT

Of the three fields with which I shall be concerned here, the first contains methodology of science and logic; the second, aesthetics; and the third, legal theory. In each, the procedural-material distinction has been made to play a crucial role.

Dewey has used it to explain the nature and grounds of the

propositions of deductive logic, and their relation to empirical science and its methodology. Briefly, his position is that there can be no valid distinction between deductive logic and the methodology of science. Both try to state the procedural rules which must guide the processes by which material scientific statements are accepted, if these statements are to provide warrant for judgment. There is, however, an important distinction between these procedural disciplines and science itself. Whereas material scientific statements may be justified only in terms of certain procedural rules, these rules themselves (the content of logic-methodology) can be justified only in terms of their conduciveness to the ends of scientific inquiry, namely, the resolution of problematic situations.

> . . . experience in inquiry, as in conduct of business transactions, made it evident that the purpose for which inquiry is carried on cannot be fulfilled on a wide scale or in an ordered way except as its materials are subject to conditions which impose formal properties on the materials. When these conditions are abstracted, they form the subject-matter of logic.[1]

Felix Kaufmann, in *Methodology of the Social Sciences,* uses the procedural-material distinction much as Dewey does to establish a similar relation between logic, empirical science, and methodology. He, too, wants to distinguish between

> . . . the *structure* of a science as defined in terms of the rules of procedure and the *corpus* of a science at a given time, i.e. the set of propositions accepted at this time in accordance with the rules of procedure of the science.[2]

As with Dewey, material scientific propositions are to be justified in terms of these procedural principles, but procedural principles in terms of the end of inquiry, conceived this time as the making of successful predictions. In addition, Kaufmann uses the notion of "procedural rule" to define "science" itself, and to explain the nature and grounds of various rules involved in science—such as the "ideal" laws of optics and mechanics—which have sometimes been treated as synthetic *a priori.*

In the field of aesthetics, Abraham Kaplan has proposed that the distinction between procedural and material rules may be used to resolve the apparent deadlock in contempory critical theory concerning the nature of aesthetic standards.

> What is being suggested here is that the "crisis" in criticism has been resolved by a widespread recognition that material standards are derivative from procedural standards, on which there is, in fact, considerable agreement.

[1] Dewey, John, *Logic, the Theory of Inquiry* (New York, H. Holt, 1938), p. 373.

[2] Kaufmann, Felix, *Methodology of the Social Sciences* (New York, Oxford University Press, 1942), p. 42.

The procedural standards do not, to be sure, uniquely determine a set of material standards. But whatever differences arise at any time can be removed—if at all—only by a further application of these procedures.[3]

Finally, in the field of legal theory (which I touch on here only for the light it may throw on similar problems in philosophy), the distinction between material and procedural rules has been used to account for the origin of certain rules of law—

. . . whenever we trace a leading doctrine of substantive law far enough back, we are very likely to find some forgotten circumstance of procedure at its source; [4]

to determine under which conditions overrulings of decisions shall be retrospective—

Where overruled decision deals with "procedural law". . . effect of overruling decision is prospective only, but where overruling decision deals with "substantive law," effect of overruling decision is retrospective; [5]

and to determine under what conditions "foreign law" shall be honored "at the forum"—

This attitude toward foreign law (holding it to be binding at the forum only when substantive), with its resulting tendency to give preference to local law, was calculated to give to procedure a broad content as a rationalizing device, and it is not difficult to surmise that the result to be obtained, denial of effect to foreign law at the forum, often led to a desire to broaden procedural content.[6]

These examples, moreover, provide only a narrow sample of the range of uses to which the distinction has been put.

II. Requirements Made of the Procedural-Material Distinction

All of these uses agree in imposing certain conditions on the procedural-material distinction. The following are four of the more important of these conditions.

(1) Procedural rules must be distinguished in their content and in their greater stability, from the corpus of the inquiry for which they are procedural—e.g., the corpus of legal rights, scientific decisions, aesthetic judgments. In the case of Dewey and Kaufmann, it is partly to account for the nature and grounds of such a "fixed"

[3] Kaplan, Abraham, "On the So-Called Crisis in Criticism," *Journal of Aesthetics & Art Criticism*, Vol. VII (September, 1948), p. 44.

[4] Holmes, Oliver Wendell, Jr., *The Common Law* (Boston, Little, Brown, 1938), p. 253.

[5] *Words and Phrases*, Vol. 34, p. 74.

[6] Sturnberg, George W., *Principles of the Conflict of Laws* (Brooklyn, Foundation Press, 1951), p. 155.

body of rules (deductive logic) that the distinction is made. Moreover, all the men considered require that procedural rules provide a more or less fixed standard of appeal for the resolution of material conflicts. Only if they meet this first condition can they be expected to do so.

(2) Procedural rules must be subject to a different sort of justification than material ones. This condition is stated explicitly, as we have seen, by Dewey and Kaufmann; it is implicit in Kaplan's proposal to use procedural principles to resolve material conflicts. This proposal could not be made sensibly unless material conflicts did not arise again at the procedural level—which they would do, indeed, if the grounds of procedural and material rules were identical.

(3) Procedural rules must provide a working basis for determining the correctness of certain sorts of decisions. This again is stated explicitly by all the men considered. As we shall see, they define as procedural those rules by which we determine the correctness of "scientific judgments," "scientific decisions," "aesthetic judgments," "legal judgments." However, there is room for misunderstanding here. Procedural rules do not perform this function alone. Kaplan tells us explicitly that the correctness of aesthetic judgments is *directly* determined by appeal to material rules; but that procedural rules prescribe the manner in which these are to be used and provide the only standard of appeal for conflicts of material rules.[7] This, I think, is what Dewey and Kaufmann mean, too—Dewey admits of "material means," [8] as we shall see; and Kaufmann probably takes most scientific laws as material, for he singles out only a few of them for treatment as procedural.[9]

(4) Finally, procedural rules must be capable, as a group, of eliciting certain human attitudes that material rules do not elicit. For Kaplan, there is greater "agreement" about procedural rules[10]; for Dewey and Kaufmann, we hold on to them more "persistently" [11]; for the legal theorist, we attach less "importance" to them.[12] It does not follow analytically from the definition of "procedural rule," according to these writers, that procedural rules evoke these attitudes; but however defined, they must be such as would, as a matter of fact, evoke them.

[7] Kaplan, Abraham, *loc. cit.*, p. 44.
[8] Dewey, John, *op. cit.*, p. 136.
[9] Kaufmann, Felix, *op. cit.*, p. 45.
[10] Kaplan, Abraham, *loc. cit.*, p. 44.
[11] Dewey, John, *op. cit.*, p. 13; Kaufmann, Felix, *op. cit.*, p. 156.
[12] Sturnberg, George, *op. cit.*, p. 156.

These are the major conditions the procedural-material distinction must meet in order to carry the significance attached to it. Other conditions would have to be added, of course, if we were considering not merely "procedural rules" but "procedural-rule-which-ought-to-be-followed." While all the men considered are concerned with the latter, it is their analyses of "procedural rule" generally, rather than their standards of appropriateness in procedural rules, that I want to examine.

III. Analyses of "Procedural" and "Material" Rules

We are now in a position to consider some of the ways in which this distinction has been analyzed, and to criticize these analyses both in terms of the usual standards of analysis and in terms of the conditions stated above.

It has been proposed, first of all, that procedural rules are distinguished by being "prescriptive" for inquiry. Dewey and Kaufmann both seem to take this view.

> Universal propositions about logical forms are propositional functions which are in themselves neither true nor false. They state modes of procedure in inquiry which are postulated as applicable and as required in any controlled inquiry. Like mathematical axioms, their meaning, or force, is determined and tested by what follows from their operative use.[13]

> Such laws . . . are prescriptions for scientific procedure concerned with the actual cosmos. In other words, they are not synthetic propositions but rules of procedure.[14]

And Kaufmann further implies it in the following:

> How can it be determined whether a law is a synthetic universal proposition or a rule of procedure? The answer is: it must be made clear whether the proposition is considered as falsifiable by observational test.[15]

All prescriptive rules are procedural according to this test: for no prescriptive rule is considered as falsifiable by observation of a negative instance.

If these philosophers are merely distinguishing procedural rules from their context, while presupposing a general analysis of procedural rules, all well and good. If they propose prescriptiveness as their general analysis, however, then they have said nothing which is not applicable to all rules in their prescriptive use. And in view of their various purposes neither Dewey nor Kaufmann would want to say that all rules, so used, are procedural.

It has also been proposed by all the men considered that those

[13] Dewey, John, *op. cit.*, p. 157.
[14] Kaufmann, Felix, *op. cit.*, p. 87.
[15] Kaufmann, Felix, *op. cit.*, p. 86.

rules are procedural which "lay down conditions for" or "state modes of procedure in" inquiry. This general formulation of the proposal is Dewey's,[16] but Kaufmann's view that procedural rules "state modes of procedure for determining the correctness of scientific decisions"[17] (decisions concerning the acceptance of propositions into the corpus of the science), Kaplan's view that procedural rules "state the procedure which critical inquiry must satisfy for its results to provide warrant for judgment,"[18] and the legal view that procedural rules "state the manner in which legal rights are to be enforced, or of obtaining redress for their invasion,"[19] may all be included in the proposal. In all of its forms this proposal is subject to several kinds of objections.

In the first place, many of the propositions these men want to call procedural rules do not always lay down conditions for the inquiry in which they appear: they may occur as the results of inquiry or as premises of an inference or argument in inquiry. The same rule may occur in one context as the premise of an argument within an inquiry, and in another may lay down conditions for an inquiry, without being contained in it. However, the men we have considered certainly give the impression that a rule is either procedural or not; that once procedural, it remains so. Therefore, this analysis of "procedural rule" is too strong. It excludes some of the very rules for whose explanation the distinction was introduced in the first place.

Furthermore, any rule may be rephrased to state the conditions inquiry must fulfill. "Maximize happiness" may be rendered, "In making a decision select that alternative the realization of which will maximize happiness." The former sounds as if it were about actions or happiness, the latter as if it were about inquiry. Thus, to look at the other side of the coin, this analysis would seem to be weak enough to include too much as procedural: namely, rules of any kind whatsoever. It goes without saying that, in this case, the conditions of the analysis would not be met: procedural rules would not be distinguished in content, stability, and mode of justification from material ones.

Finally, even ignoring the above, rules which are most obviously procedural may often have considerable substantive effect, i.e., a change in them may well affect the character of the decision made; and rules which are most obviously material often "guide" or constitute "conditions" for inquiry, in the sense that one is forced to

[16] Dewey, John, *op. cit.*, p. 13.
[17] Kaufmann, Felix, *op. cit.*, p. 42.
[18] Kaplan, Abraham, *loc. cit.*, p. 44.
[19] Sturnberg, George W., *op. cit.*, p. 156.

change his mode of procedure in order to deal with the sorts of premises involved. Again, this analysis would seem to nullify the distinction, making all rules—particularly, rules one wants to call material—procedural.

One way of avoiding some of the criticisms listed above would be to define as procedural only those rules which lay down conditions for inquiry terminating in a set of substantive decisions already "given" in its entirety. Thus there could be no question of a rule shifting from the substantive to the procedural realm. It is possible that Kaufmann and some of the legal theorists take this view. Sometimes they use "scientific decision" and "legal right" as if the words stood for just such a fixed and presupposed class of entities. The obvious objection to this analysis, however, is that the class of legal rights or scientific decisions is no more clearly defined than that of procedural principles. In the law, for example, there are admittedly procedural rights—e.g., the right of a defendant to request a directed verdict. How are these to be distinguished from legal ones? There must be "procedural decisions" in science; how are they to be distinguished from what Kaufmann calls "scientific" ones? My point is not that these questions cannot be answered, but that in answering them we raise the same problems the "fixed class of substantive rights or decisions" was introduced to avoid. There are other difficulties as well. The class of legal rights or scientific decisions cannot very well be said to be given once and for all in its entirety, when there is admitted to be transition from the procedural to the substantive character of rights in the law, and when it is admitted that new decisions are always arising in science, and must be characterized in each new instance as material or procedural.

In addition to these analyses, which may be attributed with some degree of plausibility to all the men considered, there are some more special ones. Dewey, for instance, at one point offers the following:

> Propositions about subject-contents, about spatio-temporal conjunctions of properties of existence, thus undergo independent development just as do propositions about meanings and their relations. The former will be called *material means*, and the latter *procedural means*, it being remembered that both are operational since they are means of determining the final situation and judgment.[20]

It is not clear whether this is intended as an analysis of "procedural rule," or of a sub-class of procedural rules. If the former, however, then I can only suppose it to be a misguided result of Dewey's desire to include the propositions of deductive logic as procedural

[20] Dewey, John, *op. cit.*, p. 136.

for science. First, this test conflicts with the test by which procedural rules are said to "lay down conditions for inquiry." Not all propositions which lay down conditions of inquiry are about meanings and their relations—e.g., "Don't seek information from drunken authors." If this test is to be used alone and the other abandoned, then the conditions of the analysis, mentioned in Section II, are not satisfied. For in cases where the justification of substantive propositions can be provided, it is not always provided by principles which on this analysis are procedural.

It is proposed by some legal theorists, and by no one else as far as I know, that those rules are procedural which "do not affect the rights involved" or "do not affect the outcome of the case." [21] However, every rule which comes into a case at all may affect its outcome. It is certainly rash to predict that the circumstance governed by a particular rule will never be just that circumstance on which the outcome of the case will depend. If rules are said to be procedural just insofar as they do fail to affect the outcome of the case, however, then—while the distinction seems to me applicable—it is no longer a distinction which concerns procedure. It is simply the distinction between important rules and trivial ones, functional and non-functional ones. This distinction may be, indeed, what the legal theorists in question are getting at. But a rule which concerns procedure may as easily be trivial or important as one that does not.

Thus, of all the analyses considered, none has been found which both provides an applicable test and meets the conditions necessary for the procedural-material distinction to retain the significance assigned to it.

IV. Proposed Distinction

The procedural-material distinction I want to propose has been suggested by almost all the men considered above, and presupposed, though not explicitly stated, by at least two of them (Dewey and Kaplan). I would make the distinction hinge on the function, rather than the content, of the rules involved. Procedural rules guide decision-behavior; material rules, decisions. What must be explained now is the meaning of "decision-behavior" and the manner in which rules may be said to "guide."

Decisions and Decision-Behavior. I shall use the word "decision" to designate that act or event by which an individual commits himself or registers his intent to undertake a certain course of conduct. For any given decision, the individual's "decision-be-

[21] Sturnberg, George W., *op. cit.*, p. 156.

havior'' is the series of operations which lead up to, result in, or affect that decision. It is here, first of all, that I want to insist on the functional nature of the procedural-material distinction. ''Decision-behavior'' is a functional term. That means, in the first place, that it is a relational one. A series of acts or operations is decision-behavior only *for* a given decision. Secondly, the ''same'' set of acts or operations may be decision-behavior at one time, or from one point of view, and not at or for another. Consider my decision to go to the movies tonight, and the act of calling up the theatre to find out whether the time of the showing will be convenient. In order for that act to be decision-behavior for that decision, it must (1) actually have a function in the making of that decision, and (2) be considered in terms of that function. (It goes without saying that, while both conditions must be satisfied, the second cannot be satisfied unless the first one is, whereas the second is not essential to satisfaction of the first.) Let us assume that the call does function in the making of the decision—that the decision would be different if the call were not made. Then the call does constitute decision-behavior for that decision if considered in terms of that function.

Guiding as a Rule. When we say that a rule ''guides'' the making of a certain decision, or guides the processes leading up to that decision (its decision-behavior), we may mean one of two things. We may mean, first, that the rule is an empirical generalization for which the decision, or the decision-behavior, is a confirming instance. In such a case, we say that the decision or decision-behavior ''conforms'' to the rule. Or we may mean that the decision, or the secondary decision to adopt one or another kind of decision-behavior for the primary decision, is made by ''appeal to rule.'' This ''appeal'' is a rather complicated affair.[22] In order for a decision, primary or secondary, to be made by appeal to a given rule, at least this much has to be true: (1) the rule is one to which the decision, if made, would conform, and (2) the fact that the rule ''came to mind'' in the course of the individual's inquiry was a partial cause of the decision's being made. When I speak of a rule ''guiding'' decisions or decision-behavior, I limit myself to one or the other of these things.

To recapitulate, then, a rule is material for a given decision if and only if the decision is made by appeal to or in conformity to that rule. A rule is procedural for a given decision if and only if one or more of the processes which lead up to and result in that

[22] Cf. Wessell, Andrew, and Schon, Donald, ''Appeal to Rule,'' this JOURNAL, Vol. LIII (1956), pp. 495–502.

decision, insofar as they are considered as performing that function, conforms to that rule; or if the secondary decision to adopt one of those processes rather than another is made by appeal to that rule. My treatment of the procedural-material distinction is similar to those we have been discussing, then, in concentrating on the distinction between decision and procedure for arriving at decision. It differs from them, however, in making that further distinction a functional and relational one, and in specifying further the manner in which a rule may be said to guide one or the other.

It may help, in clarifying my position, to specify in more detail the operations one would have to go through to determine whether or not a given rule is procedural for a given decision. What one must determine, first, is the sort of operations which would conform to the rule or which might be made by appeal to the rule. Then one must determine whether those operations are decision-behavior for the decision in question. I shall illustrate both steps by two cases.

Let us first consider the decision of a senate committee to table a bill, and the rule "Do not interrupt a speaker unless you are recognized by the chairman." Now the decision itself was not made in conformity to or by appeal to this rule: it could neither be a confirming nor disconfirming instance of the generalization contained in the rule. But the case is otherwise with the actions of the committee which led up to the decision. And these actions are decision-behavior for the decision to table the bill. Therefore, the rule is procedural for that decision.

Consider, to take a more difficult case, the decision of a country doctor to keep his commitments to his present patients, rather than to attempt to earn more money. And let us take the Principle of Utility—in the form where it enjoins us to maximize happiness—as our rule. The case appears difficult because the Principle of Utility may easily be rephrased in terms of procedure: "To choose between subordinate policies, determine which one will tend to maximize happiness." Nevertheless, it is still true that it is the doctor's decision, rather than the operations leading up to it, which might or might not conform to the rule; and hence, if the rule functions at all with respect to that decision, it functions materially.

V. Testing the Proposed Distinction

We must now determine whether my version of the procedural-material distinction meets the objections which may be leveled at the other versions, and still serves the ends to which certain philosophers and legal theorists put it.

Let us first consider those criticisms which applied nearly equally to all the men considered. Since "prescriptiveness" and "givenness" are no longer put forward as tests of the procedural character of rules, but are merely recognized as being essential to "rules" in the first place, the criticisms directed at those tests are no longer appropriate.

It was urged against the men considered in the first section of this paper that, while they called procedural those rules which laid down conditions for inquiry as opposed to the results of inquiry, the very rules they called procedural could figure as the results of inquiry, and the very rules they called material could guide the performance of other inquiries. This criticism, too, is no longer appropriate since a rule is held to be procedural or material only *for* a given decision. It is perfectly acceptable to my version of the distinction that a rule should function procedurally in one context and not in another.

More nearly applicable to the present view, perhaps, is the criticism that follows: it is held that procedural rules "guide the course of inquiry," but rules we would not be willing to call procedural also affect the course of inquiry; and rules we do call procedural often have considerable substantive effect. It was to meet this criticism that my explanation of "manner of guidance" was made. The point is that, while material rules may influence procedure, and procedural rules may influence the sort of decision made, they do not do so in the manner peculiar to *rules*—i.e., they are not rules by appeal to which or in conformity to which the procedure, or decision, as the case may be, is made. Just how substantive rules do manage to produce their procedural effect (and procedural rules their substantive one) is an interesting question, but one we need not attempt to answer in order to handle the objection at hand.

Finally, I held in Section I that, whereas procedural rules are sometimes defined in terms of substantive rules, substantive results, or substantive rights, these latter terms are in fact no clearer than the first. No attempt is made to make explicit the bounds of the class of substantive entities. On the present view, however, this attempt is not necessary; indeed, it is difficult in principle, since it is recognized that rules which are substantive in one context may not be so in another. All that is required is that a given decision be identified. It is in terms of that decision, then, that the procedural or material character of rules is determined: one decision may serve as well as another as the "locus" for this determination. A definition of "procedural rule" in terms of a

fixed class of substantive rights or substantive results is no longer attempted.

The question may well arise, however, whether I have avoided certain difficulties in the old manner of making the distinction at the price of foregoing those ends for which the distinction was originally intended. That this is false I shall now try to show. There were several purposes for which the procedural-material distinction was employed. It was ascertained that, in order to serve these purposes, the distinction between material and procedural rules had to meet certain conditions.

There had to be a more or less fixed body of procedural rules for a given area of inquiry. Here at once a major difficulty for my view would seem to present itself. I have said that the "content" of a rule is not a sufficient condition for its being procedural, that a rule may be procedural in one context and not in another. Would it not be self-contradictory, then, for me to assert that in certain areas of inquiry, such as science and art criticism, there may be more or less fixed sets of procedural rules which govern rational inquiry? The answer, I think, is No. While the procedural rules governing certain areas of inquiry are not constituted as such analytically by their mere content, still it may well be (in all consistency with my view of the distinction) and is as a matter of fact true that rules of a certain content are more likely to function procedurally for a given sort of decision than are other rules. Thus, to attempt to list the basic procedural principles which govern rational inquiry in science or in art criticism is on my view by no means futile (as it would be, on the other hand, to list all the decisions for which rules might be procedural). It must be remembered merely that the procedural character of these rules for this sort of decision depends on their functioning procedurally for these decisions. Should these cease to function in this way, they would cease to be procedural rules for these decisions. Thus, their "fixity" depends on a matter of fact—their function—and cannot be established before hand by a mere analysis of their content. But I do not think Kaufmann, Dewey, or Kaplan would quarrel with this last remark. There is the further point that, while procedural rules for a given sort of inquiry may themselves come under inquiry, they need not thereby cease to function procedurally for the first sort of inquiry. The rules which are procedural for science or art criticism may still be "fixed" in the desired sense, even though they may come to function materially in an inquiry other than the one in question.

It was further required of procedural rules that they be the object, more or less consistently, of certain sorts of attitudes. They had to be "held more persistently," "agreed upon more widely," etc. There is no reason why this should not be true, on my view as well. Rules which, because of their content, come to function procedurally for a certain sort of decision, may well elicit more or less consistently certain sorts of attitudes. Here again, however, the fact that certain attitudes are held toward them is not analytic of their being procedural; it is simply the way things happen to be. As far as the ends of the distinction are concerned, my analysis puts no obstacles in the way of certain attitudes being held toward procedural rules for a given sort of decision. The reason why these attitudes are held, and whether they are held, must always be made out in the particular case.

It is required that procedural rules provide a working basis for the selection, rejection, or modification of certain sorts of decisions. This, I take it, is provided by the fact that on my view the decision-behavior in question must be a confirming or disconfirming instance of the empirical generalization contained in the rule which is procedural for that decision-behavior, together with the fact the decisions are arrived at by such decision-behavior.

It is required, finally, that there be a distinction in the modes by which material and procedural rules are justified. Given my view of the procedural-material distinction, this means that the mode of justification to which a rule is subject varies with its function. It is quite possible on my view that a rule, retaining its content, would be justified in one way in one context, in quite a different way in another. In determining the appropriate mode of justification we would consider content merely as an indication of the sort of function the rule is likely to be performing. My view of the distinction does not prevent the requirement of "distinction in mode of justification" being met, so long as one admits that a rule of the same content may have to be justified differently according to its different functions.

Thus I have tried to show that the distinction between procedural and material rules, though untenable in the way it is usually explicitly made, can be reformulated in a way that is free from obvious difficulties, at any rate, and still adaptable to the main purposes for which it is used in current philosophy and legal theory.

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COMMENTS AND CRITICISM

ON THE ANALYSIS OF CAUSALITY

IN a recent issue of this Journal,[1] Professor Arthur Pap calls attention to the fact that critics of the regularity theory of causation have more than once pointed out that two events may be "constantly conjoined" without being causally connected, but have mostly been silent as to an alternative analysis of the concept of causation. He then mentions as an exception the analysis of it offered in Chapters 8 and 9 of my book, *Nature, Mind, and Death.* But he goes on to say that that analysis seems to him completely untenable, for reasons he then proceeds to set forth.

If those reasons indeed invalidate it and no valid other analysis of the concept is in sight, then, in view of the ubiquity and importance to us of the Causality relation, there can hardly be a more pressing task in the whole field of philosophy than that of formulating an adequate analysis of that relation. And to do so ought not really to be very difficult, for the notion of Causality is used many times every day by each of us, whether explicitly or implicitly. All such verbs as "to break," "to bend," "to heat," "to kill," "to twist," "to melt," "to prevent," "to steer," "to remind," "to irritate," etc., are verbs of causation. Moreover, in the majority of cases, we seem to have little or no difficulty in identifying concrete instances of the causal relationships those verbs designate as between events of the kinds respectively concerned. Hence we must have an adequate working notion of Causality, and it should be possible to analyze it.

The difficulty which has chiefly stood in the way of reaching an analysis of it that would stand up has been, I believe, that instead of attending to the obvious, the searchers have gratuitously assumed that Causality must be something recondite and therefore hard to detect. But what in fact was needed for the task was not superior ingenuity, but ingenuousness—the ingenuousness which, in the familiar story of the courtiers vying in admiration of the king's subtly fine garments, enabled the innocent child to perceive that the king simply was naked!

I believe that Pap's criticisms of the analysis of Causality I offered can be shown to be based only on certain misapprehensions. Before turning to this, however, and irrespective of the correctness or incorrectness of Pap's surmises as to how I arrived at that analysis, it will be well to state briefly what it is.

[1] "A Note on Causation and the Meaning of 'Event,'" Vol. LIV, No. 6, March 14, 1957, pp. 155–159.

1. *The Causality relation.* What I contend is that Causality is *the relation which obtains between the three terms of a perfect experiment*—the relation, namely, between a given *state of affairs* S and two *changes* (whether simple or complex) in it that are the *only* two changes in it: one of them a change C at a time T1, and the other a change E at an *immediately sequent* time T2. To say that this triadic relation obtains between S, C, and E is to say that C in S is etiologically both sufficient and necessary to E in S (i.e., both *cause* of and *condition* of E in S); and, conversely, that E in S is etiologically both necessitated by and contingent upon C in S (i.e., both *effect* of and *resultant* of C in S). This definition of Causality, being framed in terms of but one occurrence of the sequence in S of C and E, does not require the supposition that it ever occurred before or ever will again. But it entails that whenever, if ever, S, and C in S recur, then E in S also recurs.

2. *Pap's first criticism.* Pap writes: "Ducasse overlooks that the observation of a solitary change preceding the event to be explained may not be the sufficient ground of the causal judgment." The ground which Pap says may be required and actually employed in addition is: (i) elimination of alternative explanatory hypotheses—e.g., in the instance of the parcel that glowed when I touched it, the hypothesis that the parcel's continuing contact with the table was the cause; and (ii) the assumption that causality is universal—"cause" being conceived as regular immediately antecedent event.

Now, as regards (i), what Pap apparently overlooks is that, simply under the definition of Causality stated in Sec. 1, i.e., *without any tacit additional premises* such as he suggests, the supposition that C (my touching the parcel) was the *only* change immediately preceding E (the parcel's glowing) automatically eliminates as alternative the hypothesis that some *other* change could be the cause of E. And further, the hypothesis that the *continuing* contact of the parcel with the table might be the cause of the glowing is automatically eliminated too, as alternative explanatory hypothesis, by the definition's requirement that the cause be a *change* in the given, otherwise continuing, state of affairs S.

As regards (ii), since, admittedly, some cases of regularity of sequence exist that are *not* cases of causation, regularity of sequence cannot, merely as such, be what Causality consists in. Hence to suppose that "Causality," conceived as merely regularity of sequence, is universal, is to suppose that something, *which does not constitute Causality,* is universal. And such a supposition is not needed for, nor is it relevant to, the conclusion that if C (my

touching the parcel) was indeed, as my analysis of Causality requires, the only change occurring in the situation S immediately before the change E (the glowing), then C was what caused E.

Whether the only change one *observed* was the only change that *really occurred* at the time in the given state of affairs is of course a different question. What can be said about it is only that, the more carefully we look for additional changes without finding any, the more *probable* it becomes that there were no others. To analyze the concept of Causality is one task; and to identify empirically concrete exemplifications of it is another task. The outcome of the latter always remains theoretically precarious in some degree. But this is nothing peculiar to Causality. Rather, it is a feature of every attempt to identify in experience concrete cases of something one has defined abstractly—for example, Straightness, or Equality, etc.

3. *Pap's criticism under his (a) heading.* Pap states there that, on my analysis of Causality, "it is self-contradictory to suppose that an event which is immediately preceded by more than one change in its neighborhood is caused at all." But what really follows from my analysis is only that the change in S, which immediately preceded the event, was then a *complex* change; for in order that at a time T a change C in S be the *single* change there, it does not have to be a *simple* change.

Pap writes next: "If one antecedent event is causally irrelevant to *e*, then all of them are, since a change which is one of several concurrent changes in S cannot be said to be the only change in S." But any appearance that that conclusion follows is due only to the ambiguous phrase, "causally irrelevant to," which invites confusion between etiologically *sufficient to* (i.e., "cause of") and etiologically *necessary to* (i.e., "condition of"). That a complex change *abc* in S, which was the single change immediately preceding a complex single other change *def* in S, was sufficient to cause it, entails that it was *sufficient* to cause all the parts of *def*, but does not entail that the *whole* of *abc* was a *necessary* condition of, say, occurrence of the part *d* of *def*. For no contradiction is involved in supposing, for example, that, had the part *a* of *abc* been absent, the remainder, *bc*, would have been sufficient to cause, say, *df*.

The only way to find out whether or not it would have been sufficient—i.e., the only way to isolate sufficient cause from necessary condition—is to take a *new case* of the S state of affairs, but one in which the single change which occurs in it first is not *abc* but only *bc;* and then to observe whether or not *d* is again a part of the change which immediately follows in S. If it is, then the

part *a* of the change *abc* was *not necessary* to the part *d* of the ensuing change in S—and this notwithstanding that the *whole* of *abc* in S was *both* sufficient and necessary to the *whole* of *def* in S.

4. *Pap's criticism under his* (*b*) *heading.* Pap's contention, under the (b) heading of his criticism, is that my analysis of Causality would make the words "cause" and "effect" applicable only to concrete events, whereas "in any ordinary and significant use" those words "are applied to instances of definite kinds of events, not to what Ducasse calls 'concrete events.'"

What invalidates this criticism is the false assumption, on which it is based, that the *instances* of given kinds of events do not consist of concrete events. The truth is, on the contrary, that every state of affairs that exists, and every change in it that occurs, is fully concrete, no matter of what kinds that state of affairs and that change in it may respectively be instances. And of course, only changes *that occur,* in states of affairs *that exist,* can cause or be caused by anything. This is the sense in which it is true that my analysis of Causality makes the words "cause" and "effect" applicable only to concrete events; and this restriction constitutes not a defect but a virtue of that analysis, for it is a requirement which any correct analysis of Causality would have to satisfy.

That restriction, however, does not in the least stand in the way of either of the following two things:

One is—as pointed out in Sec. 3 above—that it is possible *to interest oneself* specifically in some concrete *part* of a given concrete occurrence, and to inquire as to what concrete *part* of the cause of that occurrence was sufficient to, and what concrete *part* of it was not necessary to, occurrence of that specific concrete part of the given total occurrence.

The other thing that is in no way precluded by the restriction mentioned is that one should choose to limit *the interest one takes* in a given concrete change, that is occurring in a given concrete state of affairs, to the fact that the change happens to be of a certain kind K, and the state of affairs in which it takes place, of a certain kind Z. If so, one is then simply *uninterested* in the characteristics, additional to those definitive of kinds K and Z, which the given concrete change and concrete state of affairs do possess notwithstanding one's disinterest in them—which additional characteristics differentiate from all other instances of K and of Z *the* individual instance, respectively of K and of Z, which the given concrete change and the given concrete state of affairs respectively constitutes.

Hence, when one asks for the cause of the given concrete change

of kind K which took place in the given concrete state of affairs *of kind Z*, one is correspondingly *interested only* in discovering what *the kind Q* is, of which that concrete cause, like the concrete causes of the other concrete changes *of kind K* in other concrete states of affairs *of kind Z*, was an instance.

In the light of these remarks, it becomes evident that my analysis of Causality is perfectly consistent with the meaning of such a question as "What caused this breakage of this window?" and of such an answer as "The impact of this brick caused it."

Also, statements of causal laws, which are what enable us to control or to anticipate events, can be interpreted without difficulty in terms of my analysis of Causality. For example, the rough causal generalization: "Throwing bricks at windows ordinarily causes them to break" would be explicated as: "Any concrete change that is an instance of the kind 'throwing a brick at a window' does, under concrete circumstances that are an instance of the 'ordinary' kind, cause a concrete change that is an instance of the kind 'breakage of the window.' "

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BOOK REVIEWS

Der Rang des Geistes; Goethes Weltverständnis. FRITZ-JOACHIM VON RINTELEN. Tübingen: Max Niemeyer Verlag [1955]. 436 pp. DM 29.—.

Germany gave birth to her most balanced minds and geniuses after orgies of passion, the bloodshed and the devastation caused by the Thirty Years' War. Leibniz, Johann Sebastian Bach, Lessing, Kant, and, finally, Goethe were all children of the hundred years following the calamitous struggle that had taken place in that country in the seventeenth century. In fact, the inner harmony and poise of Goethe's nature may often have been overestimated. Still, in the parallel intellectual situation with which present-day Germany is confronted, after the excesses of passion in the two world wars of the twentieth century, Goethe seems especially destined to become the great helper in the transition from a fierce philosophy of tragedy, of a pitiless being exposed to the world, "thrown into the world," "Geworfenheit" (Heidegger, 1928), to a new philosophy of "Geborgenheit" (Bollnow, 1953), of "being sheltered."

The first and still leading protagonist of this profound change in present German thought and feeling is Fritz-Joachim von Rin-

telen who, since 1947 in his *Dämonie des Willens,* 1948 in his *Von Dionysos zu Apollon,* 1951 in *Philosophie der Endlichkeit als Spiegel der Gegenwart,* and now in *Der Rang des Geistes,* offers extremely valuable and learned thought material for the opposition to the demoniac and heated philosophizing represented by German and French existentialism. It is most gratifying to see how, despite the vast and high-ranking literature on Goethe built up in the last hundred and fifty years, Goethe can still fulfill a new mission if freshly viewed by a devoted thinker who has been deeply and rightly shocked by the turmoil and the brutal aggressiveness of the Ludendorf and Hitler regimes and their "philosophical superstructures."

Von Rintelen does not, of course, overlook the fact that Goethe is primarily a very great lyricist, perhaps the greatest of modern times, and that he was neither a professional thinker nor a classicist throughout the whole of his long life. In the one hundred forty volumes of his writings there are only scattered epistemological observations, and there is, in his youth, Promethean emotionalism and Werther's storm and stress, of which the maturer classical poet no longer approved; and again, in the last few decades of his life, classical thought is not unmixed but is rather interwoven with mystical and romantic feeling. But von Rintelen admirably succeeds in depicting the classical gist of Goethe's ethical and general *Weltanschauung* and its connection with, as well as its differences from, Christian tradition, the speculative and romantic thought of the nineteenth century, and present-day existentialism.

Thus, by a wealth of information and penetration in the analyses of philosophical trends of the last two hundred years, von Rintelen's work, in my opinion, surpasses such well-known recent studies on Goethe's philosophy as those of Simmel, Rickert, and Kurt Hildebrandt. And the work certainly gives most reassuring evidence of the vitality and the philosophical maturity of those German thinkers who had the stamina to defy Nazi ideology even at the time of its greatest triumphs.

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Our Cultural Heritage. THEODORE MEYER GREENE. Houston: The Elsevier Press, 1956. xii, 257 pp. (The Rockwell Lectures, The Rice Institute, Houston.) $3.50.

The heritage, present traits, and future possibilities of a national society require constant re-assessment from generation to generation, and at every possible level of interpretation. Our

American society is becoming richer annually in the works by our thinkers—and by foreign thinkers as well—which present many methods of approach to this task.

Greene has telescoped a variety of approaches and levels into the compass of these Rockwell Lectures. *Our Cultural Heritage* is a fusion of the closely analytic with the highly synoptic; of the historical and philosophical with the psychological and moralistic. Yet with all this variety and profundity within a short range, the author has, in the opinion of this reviewer, achieved a remarkable unity that is never forced, since it is written with catholicity, humility, and an admirable simplicity.

As he attempts to expound and then evaluate the four basic cultural resources of our society (Hellenism, the Hebraic-Christian Faith, Modern Science, and the Democratic Experiment), Greene employs essentially two cardinal principles of classification and evaluation: viz., the classificatory triad of naturalism-humanism-theism, and the evaluative triad of process-person-piety. He then applies these two triads to each of the four great streams of our cultural heritage, with vigor, subtlety, and comprehensiveness. And in the application of these triads, the author leaves no doubt of his profound faith in the supreme value of theism and piety as the ultimately regulative principle of his world-view and life-view.

Given his positive theism, Greene is disturbed by the disconnection between "the God of the Philosophers" and "the God of Faith," and he expresses this concern in perhaps the most pointed remarks in his book, under Section a, in Chapter II.

This book, excellent in its clarity of exposition and persuasive in the earnestness, range, and intelligence of its conviction, is a compelling contribution to our growing vision of American society. It fills a need and occupies a niche of its own, for its distinguishing trait is that it mediates between the primarily secularistic accounts on the one hand, and the accounts of theologians on the other hand. In a word, Greene's philosophic faith is a purged and disciplined theistic liberalism, and he has rendered, under this image, a notable interpretation.

FREDERIC HAROLD YOUNG

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